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REPORT 469

FUTURE RESEARCH ON NOISE

A ROUND TABLE DISCUSSION

Edited by

JEAN J. GINOUX

APRIL 1963

NORTH ATLANTIC TREATY ORGANIZATION

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This Report is one in the Series 448-469 inclusive, presenting papers, with discussions, given at the AGARD Specialists' Meeting on 'The Mechanism of Noise Generation in Turbulent Flow' at the Training Center for Experimental Aerodynamics, Rhode-Saint-Genèse, Belgium, 1-5 April 1963, sponsored by the AGARD Fluid Dynamics Panel

SUMMARY

This Report consists of a round table discussion held on the last day of the Specialists' Meeting on 'The Mechanism of Noise Generation in Turbulent Flow' sponsored by the AGARD Fluid Dynamics Panel, at the Training Center for Experimental Aerodynamics, Rhode-Saint-Genèse, Belgium, 1-5 April 1963. It comprises a series of questions and answers between members of the Panel themselves, followed by an open discussion between the Panel and members of the audience. It concludes with statements by members of the Panel of intentions regarding future work in this field.

SOMMAIRE

Ce rapport consiste en une discussion pour échange de vues qui s'est tenue le dernier jour de la Réunion des Spécialistes sur 'Le mécanisme de la production du bruit dans l'écoulement turbulent', organisée par le Groupe AGARD de la Dynamique des Fluides, au Centre de Formation de l'Aérodynamique Expérimentale, à Rhode-Saint-Genèse, Belgique, du 1er au 5 avril 1963. Il comprend une série de questions et réponses échangées entre les membres du Groupe mêmes, suivie de débats libres entre le Groupe et des personnes de l'assistance. Il se termine par des déclarations, des membres du Groupe, sur les intentions relatives aux futurs travaux dans ce domaine.

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LIST OF PARTICIPANTS

Moderator: Prof. E.J. Richards (University of Southampton, Southampton, England)

Panel Members: Prof. A.J. Favre (Inst. Mec. Statist. de la Turbulence, Marseille, France)

Dr. J.E. Ffowcs Williams (Bolt, Beranek and Newman, Inc., Cambridge, Mass., U.S.A.)

Prof. I.E. Garrick (Langley Research Center, Hampton, Va., U.S.A.)

Prof. L.S. Kovasznay (The Johns Hopkins Univ., Baltimore, Md., U.S.A.)

Dr. J. Laufer (J.P.L., Pasadena, Calif., U.S.A.)

Prof. G.M. Lilley (College of Aeronautics, Cranfield, England)

Prof. A. Powell (U.C.L.A., Calif., U.S.A.)

Prof. H.S. Ribner (University of Toronto, Toronto, Canada)

Dr. J. Sternberg (Martin Co., Baltimore, Md., U.S.A.)

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A ROUND TABLE DISCUSSION

5 April 1963

INTRODUCTION

by

Prof. E.J. Richards

Gentlemen, I do not think I need to introduce the panel because we have been discussing things for the whole week, but I think it would be worthwhile to outline the approach that we are going to take during this session. Apparently, when round table conferences of this kind have occurred in the past and the audience has been brought into the discussion the result has always been that they do all the talking and the round table does nothing.

Since we have, on the stage, the people who have been working in this field for a very long time, I think that this would be a pity, and so we are going to confine the discussion, at least for the first part of the meeting, to the table itself. I am going to pick on topics which I feel are the important ones and the ones which should be ironed out. Then, if we run out of topics, we will call on the members in the hall to ask questions.

There are so many topics that it is almost impossible to know where to start. But in view of the fact that so much of the work this week has been discussing space-time correlation and since Professor Favre is the originator of this sort of technique, certainly in turbulence investigations it might well be worthwhile to start on the experimental side. What, in fact, can be measured in space-time correlation techniques, what more is needed to build up our knowledge of understanding of turbulence and are we likely to get anything from this understanding which will help us in our noise studies? I would like to stick, in general, to this topic and I call on Professor Favre to introduce it.

QUESTION 1

What can be measured in space-time correlation techniques? What more is needed to build up our knowledge of understanding of turbulence? Are we likely to get anything from this understanding which will help us in our noise studies?

Prof. Favre

Thank you, Professor Richards.

It is a long time since we started to do some measurements on space-time correlation. During the war we started with an old magnetic tape recorder and since 1948 we have had good magnetic tape recorders working and making measurements in turbulence. So we acquired a lot of measurements of double velocity space-time correlation in some simple flows. For five years we worked on the conventional isotropic homogeneous turbulence behind grids and then we made our main discovery. I mean the check and extension of Taylor's hypothesis.

I refer to the frozen pattern of eddies which can be followed along the mean movement with time delay. This was a first approximation and involved obtaining the statistical history of turbulence and showing that the turbulence has a very long life, which is due mainly to the big eddies which have a long heredity.

Then for five more years we did the same thing in boundary-layers with zero pressure gradient and we tried to apply Taylor's hypothesis in the shear flow. We found that, to a first approximation, it was valid, but that in the second approximation the shear was having some effect.

We also did the same thing for a boundary-layer with an adverse pressure gradient. Here the fundamental phenomenon is exactly the same. Recently we measured the space-time double correlation, as I showed you in the pictures, with a narrow band pass.

We found that, for a second approximation to Taylor's hypothesis, it was not really a frozen pattern which was carried on by the mass flow and that there were variations of convection velocities with the size of the eddies and the magnitude of the shear.

Fortunately, for several years now, many laboratories have been doing good experimental work in the noise field. This is especially true in the study of space-time correlations with pressure at the wall and outside the boundary-layer at low speed and at supersonic speed. They have also been making measurements of pressure at the wall and velocity in the boundary-layer.

All this giving very important information.

The question now arises: what measurements have still to be done? There is one difficulty. Logically we should programme to measure all the terms of all the equations governing the phenomenon in all the flows of interest. But if we consider the equations that give the triple and quadruple correlations, that is the pressure-velocity-velocity, the pressure-velocity-velocity-velocity and so on, in three dimensional space with time this makes a tremendous quantity of measurements.

So we need a careful selection of the measurements to be made. As we are not numerous enough in laboratories, people and facilities to make all the measurements, it seems to me that although we do not need to have an overall plan for research (we must, individually, have complete freedom) we should have some cooperation in order to avoid overlap of measurements. Sometimes this is a good check, but often it wastes time, energy and money. We have to discuss what kind of measurements are required and which could be done by each one of us.

It seems to me that the triple correlation must be measured. We began, two years ago, to measure the space-time triple correlation of velocities in some cases. However this was a very unwieldy programme because the triple velocity tensor has 27 components and it is easier to consider the noise field because pressure has the dimension of a scalar, and not a vector, and the triple correlation pressure and velocity and velocity has only nine components.

But all this work was very unwieldy and it seems to me that it would be of interest now to measure the pressure-velocity-velocity space-time correlations and to continue with the velocity-velocity-velocity space-time correlations.

I should like to hear the opinions of the other members on this topic.

Prof. Richards

There are so many measurements to make that we have obviously to sort out which ones are the most likely to lead us to worthwhile results. Presumably we should discuss this from the point of view of which measurements are going to lead to an understanding of the radiated sound.

I think that a little later on I would like to discuss experimental techniques; but who would like to start discussing which of these various correlations we should go for?

Prof. Kovasznay

When you are faced with this flood of data a certain amount of conceptual clarification is called for. Most of you know that it took quite a while before people studying turbulence really digested the Fourier transform relationship between correlation and power spectrum. Now as soon as space-time correlation is brought in, the picture becomes rather less clear, because when space-time correlations in turbulence measurements were initiated the attention was focussed primarily on the validity of Taylor's hypothesis.

Of course, there is a concurrent problem. If the hypothesis is not exactly true, what does the falling off and broadening of the correlation curves mean? We really can say there is an average life time of the eddies or we can become very formal and say we have both a variable transport velocity and a spectrum of transport velocity.

I am thinking here principally of the boundary-layer case. In a fully developed turbulent boundary-layer one has homogenity in the x direction or at least, to the degree that we are interested, we may assume this, since the rate of boundary-layer growth is not so spectacular. Now if one has homogeneity in the x direction, then the power spectrum is the same at all points. The only thing that happens between two stations is that the phases get scrambled. Essentially, a phase mixing occurs

and this phase mixing should be conveniently expressed. What I propose is illustrated in Figure 1, at the end of the Discussion. In this figure τ is the delay and R is the correlation which is something like (A) for zero separation. Then we have an envelope (B) and the customary curves (C) occur, which become fatter as τ increases.

Now let us propose a very simple model: I am considering a single scalar variable in one dimension. If we think of phase mixing we should not forget that the power spectrum of the signal is the same everywhere. We can imagine that there is a continuum of phase velocities with some kind of spread and a phase change occurs as τ increases. You could compute a transport velocity spectrum from this.

Let us suppose that you have a continuous range of velocities by which the phases are transported and that this range is spread by some spectral distribution. From this you can compute a consistent picture of the rate at which the phases are spread or the kind of transport velocity spectrum you have.

What I really propose is that we digest good existing data in this light. This has not yet been done.

Dr. Ffowcs Williams

I think that the approach Professor Kovasznay is suggesting is really the same as Wills has presented in his Report. He has given measurements of the technique of how you can represent frequency by the wave number-velocity products and how the loss of correlation is, in fact, given by a phase mixing effect. So here we have the technique already in use.

I would like to go on to make a comment about some of Professor Corcos's work. This is related to a point Professor Favre made: that if we have to go and look over old ground in this three-dimensional work then there is an enormous amount of measurement to be done. Perhaps the best thing we could hope to get out of experiment would be a model where we could reduce the number of points which we would have to study in order to get a knowledge of a three-dimensional system; and Professor Corcos has, in fact, given a model where you can obtain a two-dimensional spectrum from a one-dimensional function. I think that is exceptionally valuable.

Dr. Laufer

Of course one would like to end up with a model of this sort. But first we need measurements, at least in one part of the shear layer, which enable one to map out completely the wave number-phase velocity plane. (I prefer to use wave number rather than frequency.)

Prof. Richards

But do you think that you are really going to get a model of any significance just by taking it at one place?

Dr. Laufer

Maybe not at one point only, but at several points across the shear layer. From what I know of Professor Corcos's initial calculations, they seem to be very promising

within a certain range of his parameter. He took the actual experiments of Professor Willmarth and using a similarity assumption produced some very reasonable results.

Dr. Sternberg

I believe that you have a situation where you have a set of experimental data which can probably be represented in various ways. But the actual situation is perhaps too complicated to expect a very simple theory to explain everything. For example, Professor Favre's measurements showed that as you move through the shear layer the variation of the wave velocity as a function of scale (wave number) changes.

But since this varies from point to point, and since when you are measuring at a point you are not measuring truly local conditions (you are measuring things that are affected by what is going on elsewhere), it is not clear to me how you can get a very simple clear-cut theory of the kind that Professor Kovasznay was talking about. Would you want to comment on that?

Prof. Kovasznay

I was not proposing a theory. I was proposing that we should discover how to crystallize these data into a well defined picture. What you have said is correct because it is probably the vorticity which is the most prominent feature and even the induced velocities have this non-local character as they are being induced by the surrounding vorticity. The pressure is even more badly affected. From vorticity to velocity you need one spatial integration; to get pressure you need a second integration.

Dr. Laufer

Do you suggest, for instance, that we should use the vorticity rather than the velocity spectrum?

Prof. Kovasznay

If it is possible, I think it would be much more interesting to measure the space-time correlation of vorticity, which suffers far less from this non-local character. It does suffer because you have images and induced effects; but it is not as bad as velocity or what is even worse, namely the pressure.

Prof. Richards

Are you suggesting that you have to measure vorticity at every point?

Prof. Kovasznay

It can be done.

Prof. Richards

Well how would you suggest doing it? Are we measuring the wrong thing?

Prof. Kovasznay

No, but we are measuring an integrated effect. And in order to obtain vorticity you can measure $\partial u/\partial y$, and the normal component to the flow, quite well with close double wires; and you measure 'almost' $\partial v/\partial x$: you really measure $\partial v/\partial t$. But the two combinations will give you an ω_z, or something close to ω_z. This would be very valuable to measure. $\partial u/\partial y$ is measured with a double wire, and v is measured with an inclined probe. Actually there are tricks you can play in the boundary-layer by using an inclined probe with a normal wire above and below. This kind of configuration is better than the 'X' wire. This sort of thing has actually been done and by processing the signal from such a combination something that is nearly equal to the vorticity has been obtained. You would have a 'fudge' factor nearly equal to one.

Prof. Richards

Supposing you do this: then you get something which is nearer to the type of thing you are looking for. But don't you really want a theoretical model of the turbulence in the shear layer? And is there any hope of getting such a thing, something that is really applicable for a whole series of pressure gradients, curvatures, initial turbulence, etc.?

Prof. Kovasznay

Just reviewing the development of experimental verification of our ideas on models in turbulence we used Taylor's hypothesis as a very convenient crutch by converting space derivatives into time derivatives and claiming that what evolves in time really can be used as a space distribution. Now sound problems introduced a completely new element, namely that a frozen pattern doesn't give a contribution to sound. So we have to learn the degree to which the pattern is not frozen, since that is what is responsible for the sound generation. We have to understand this at least to the degree that it is understood in ordinary turbulence and this much hasn't really been done yet. We are not at the same level of understanding as we were on homogeneous turbulence using Taylor's hypothesis and explaining production, dissipation and diffusion. I am thinking, for instance, of Laufer's work in pipes and Corcos's in jets and others where the understanding is at least carried to the point where we can account for what is happening.

We were interchanging space and time very freely with Taylor's hypothesis. Now we cannot do that and so we have to obtain a theory. I was suggesting a little earlier that this linear phase mixing should be the next step and should be carried to the same degree of understanding. I think that most of the people involved in noise research want to jump to sound pressure too soon.

Prof. Ribner

It's a question of what problem you are attacking here. If you're attacking the sound from a boundary-layer then I think two points might be made. First, the sound radiated directly from the turbulence is known, in many circumstances, to be much weaker than the sound excited by vibration of the surface. Secondly, I think we already know the pressure field by means of space-time correlations in much more

detail than we are able to cope with in applying it analytically to the excitation of panels. For example, the most ambitious effort that I know of was the one of el Baroudi in attempting to use Dyer's method to calculate the excitation of a panel. He already had to idealize the experimental space-time correlations that we have been talking about in two ways. First he squeezed the hill into a delta function. This was yawed at 45°; secondly there was no variation of convection speed with eddy size. All this was omitted from the picture to make the analyses mathematically tractable. Moreover, Professor Richards has reported today quite a surprising degree of success in simplifying even more than this by forgetting entirely about the space-time correlation and treating this pseudo-sound pressure field, the wall pressure in a turbulent boundary-layer, as if it were sound waves; and then applying acoustic laws for the transmission of sound through a panel to calculate cabin noise. This is about as far from the measured pressure patterns as you can get.

So as far as boundary-layer noise from turbulence-excited panels is concerned it seems to me that we have already gone far enough in the refinement of our measuring techniques. It is our analytical application of this that needs to be refined.

Prof. Richards

I'd like to come back to the structural and boundary-layer sides a little later on. But now I would like to get an answer to your question: is it stupid to talk in terms of noise output? What you are really saying is that the research work should be aimed at understanding turbulence because as soon as we do understand it then we have a chance of getting some success in the next step.

Prof. Kovasznay

Yes, even if we understand it to the degree that we have understood the previous step, and also if we have a reasonable picture on which to draw.

Prof. Richards

Could I ask the members of the panel? I thing this is a very important question. Do you feel that there is a chance of success in studying turbulence in a shear flow, and is this what you feel we should be doing? Or should we be arguing whether it has any relationship to noise? Do the two things differ?

Dr. Laufer

I think we all agree that, as far as the sound generation is concerned, Lighthill's formulation provides an exact expression. But the big question is, once we have his volume integral, how to evaluate it? In order to express explicitly the sources in the volume integral, we have to know more about the turbulence. So we again face the difficulty of having to understand better the problem of turbulence and particularly certain new aspects of turbulence. Thus, to answer Professor Richards's question, in order to calculate the noise, it is indeed necessary to further study shear turbulence.

Prof. Kovasznay

In principle everything is measurable. The question is, do we have any short cut, so that we are not forced to measure everything?

Prof. Lilley

I would agree with what a lot of the other speakers have said about requiring certain special correlations in order to understand turbulence and sound generation. But one usually finds that the more complicated a measurement is, the less accurate it becomes. So that when you have the data, if it does not agree with one's theoretical ideas, one begins to suspect it. So I would like to go back to what Professor Favre said first of all. What is the next stage in the measurements? I would say the next stage is to do the simple things. They are going to be very complicated, especially when we think about triple correlations. One must also bear in mind that we have the problem of shear flows, such as wakes and jets, where there is no wall. But I would certainly like to see the simpler triple correlations measured, that is $\overline{pu^2}$ and $\overline{pv^2}$. I am sure we ought to be able to arrange the theory to make use of these data.

This is a challenge to the theoretical people. We put the onus on the experimenters to give us some reliable data and it is up to the theoreticians to arrange the theory to make the best possible use of these data.

However, we come back to the problem that Strasberg raised yesterday, since these pressure-velocity correlations in shear flows require the measurement of the fluctuating pressure and the question is 'how do we measure pressure?'. Can we see, in the future, any possible method of getting reliable data on the fluctuating pressure in a shear flow?

Prof. Richards

I was going to raise that point. It seems to me that pressure at the wall is a relatively limited thing. You really want the pressure at the same point in the flow as the velocity is being measured.

May we ask the panel now to comment on instrumentation in general? Is there any chance of measuring pressure accurately, assuming you can make the instruments small enough? Will we be able to get $\partial p/\partial t$, or something of this kind and correlate it spatially? Does the panel have any ideas on this?

QUESTION 2

Is there any chance of measuring fluctuating pressure accurately in the stream itself?

Dr. Ffowcs Williams

I think it is impossible, even in principle, to get at the pressure by simple probe measurements. And the reason is this: when you write down the equations defining pressure there is a term proportional to the local velocity, together with an integral. If you make the integral non-singular then the local velocity term is easily as big as the other. By putting something in to stop the flow, to sample it, you bring the local velocity to zero. So the local term is zero before you start.

Prof. Powell

The answer, then, is to have something moving with the flow, like a bubble, and to see what happens to the bubble, in water for instance.

Dr. Ffowcs Williams

All right, let me modify my statement. If you stop the flow at any one point, I doubt if it is possible, even in principle to measure pressure. But if you were able to sense it somehow without bringing in a local stopping of the flow, then my objection doesn't hold.

Prof. Richards

Surely this is a matter of magnitude. If you had a pin point size of instrument to measure the turbulence in the atmosphere...

Dr. Ffowcs Williams

Well you would still bring the velocity to zero: this is the point.

Prof. Ribner

This is a problem one has with probes in general. Anything you put in to make a measurement disturbs what it is you are measuring. However, it is possible to calibrate away the disturbance in many cases. For example, in the case of a probe in supersonic flow, the probe creates a bow wave which completely destroys what it was you wanted to measure. Nevertheless, you can determine what would have been there if the probe had not been put there. I think this is still true in the case of a static pressure probe; that it is, in principle, possible to determine the pressure that would have been there in terms of the readings that the probe gives when you put it there. True, you disturb the pressure, but you can recover the information that appears to have been thrown away.

Prof. Lilley

If it comes about that we cannot get accurate methods for measuring pressure one can always get this from the volume integral of the velocity field. So one does not

want to press this point too far. In effect, we are really getting back to the problem that we require all the information we can about the velocity field. There is still a lot more we require.

Prof. Kovasznay

Yes, if you measure quadruple correlations you get the pressure.

Dr. Davies

No experimental worker likes to suggest techniques without first having tried them himself, but if discussing ideas can be called trying, we have been doing this for some time. The point I want to put forward is nothing specific. However, measuring pressure by insertion of a probe is not the only way of doing it. It is possible to measure pressure changes either by optical or other physical techniques. Watching bubbles flow by in water, as Professor Powell suggested, is not as simple minded as it may sound. I think that the fact that one particular technique has been developed and has been very successful tends to blind our eyes to the fact that there are other physical phenomena we can use for measuring techniques. I think that we have been rather narrow minded in exploring the field of techniques that would be available.

Alternatively there are other things we can do to measure density variations; we can obtain pressure variations more easily from these than from integration of the velocity field. And the measurement techniques required are basically simpler.

Prof. Richards

Yes, but I think the big problem is getting the required accuracy. I know we tried a sort of focusing Schlieren system, to get pressure correlations, but the precision was so poor that we could not obtain the point to point correlation very accurately. I think this is the difficulty.

Prof. Willmarth

I was going to say that optical systems are interesting; but why not consider X-rays or something of short wave-length for density measurements?

Prof. Richards

Yes, you'd have to use an X-ray.

Well, are there any other suggestions on instrumentation?

Dr. Davies

I was going to suggest the absorption technique, which is the one we haven't yet looked at. We can now generate very high acoustic frequencies and the absorption properties of the gas field with which we are concerned vary with density. That is one technique and it can, I think, be quite accurate.

The other one is the optical absorption technique; we can generate a much wider spectrum of radiation from infra-red to ultra-violet and some of these wave-lengths have absorption peculiarities with which, again, we can measure density.

Dr. Laufer

I would like to point out that now we are talking about density measurements, and that is quite a different problem. As far as I am concerned I am optimistic and do not give up the possibility of measuring pressure with some sort of probe. With some ingenuity one should be able to devise a correction technique for possible probe interference.

It might be possible, as Professor Kistler and Dr. Bull have already pointed out, to measure simultaneously the output of a pressure gauge and the local v' fluctuations; the true pressure fluctuations could then be calculated. Thus the technique probably would not be a direct 'one quantity' measurement.

Dr. Sternberg

Could I make a comment? I'd like to direct this to Dr. Ffowcs Williams. I don't know very much about this subject, but I have the feeling that the question of whether you can or cannot measure the pressure fluctuation with a probe would depend on the size of the probe compared with the turbulent field at which you are looking. It seems to me that if you take the case of the boundary-layer in the atmosphere you have relatively large scale motions and it is not clear why you can t measure the local pressure fluctuation in this case.

Dr. Ffowcs Williams

If one tries to evaluate the pressure theoretically, then the fluctuating pressure, at least, comes from a fairly local region in the turbulent shear flow, such as a jet. One would be making a big mistake if one threw out the term which was about $\frac{1}{3}\rho u^2$ or $\frac{1}{2}\rho u^2$. Now if one puts a probe there, one is eliminating that term by bringing the flow to a standstill, and it appears to me that the same objections apply; but as Dr. Laufer points out, if one puts something there and then applies a correction my comment wouldn't hold. It's just that can you throw away the small term which would be an error if you brought the flow to rest without doing anything about it?

QUESTION 3

Could we gain anything by arranging an international coordinated programme between experimental turbulence workers?

Prof. Richards

There is one thing that Professor Favre said. He said there are so many measurements to make that overlapping should be prevented.

I realize that you can't organize science and that it would probably be very foolish to try to do so. But I would like to ask the members of this panel whether they think that we could gain anything by arranging programmes between the workers. For instance, it might be worthwhile now that an overall understanding of instruments is possible. Did you have that in mind Professor Favre? That, in fact, we should try and organize ourselves not to overlap?

Prof. Favre

Well I didn't think about that but I did think that one of us could record and collect the information and diffuse it to the space-time correlation workers, or something like that. If everybody who is involved or interested would write to him then he could write a small paper and give information to everybody.

Prof. Richards

But AGARD has this sort of people, doesn't it? It has a sort of correspondent or organizer in each scene.

Prof. Favre

We don't want planning; this is wrong. But giving centralization and information is different.

Prof. Richards

What do the other members of the committee feel about this?

Prof. Lilley

It is impossible to have someone measuring $\overline{pu^2}$ in one place and someone else measuring $\overline{pv^2}$ somewhere else. But I agree with Professor Favre that it can be very valuable if one has a closely-linked interchange of information, because in many of these cases one finds that there is, perhaps, something like two years between when measurements are taken and when they are analysed. Early information about such measurements could be vital for work in a given field. So it would be most valuable if we could have some arrangement of the kind mentioned above.

Prof. Richards

I think this is something that AGARD might well bear in mind.

QUESTION 4

Are the models of the structural excitation due to turbulence sufficiently known?

Prof. Richards

I would like to discuss the question of models of structures, that is the development of understanding of the response of structures due to boundary-layer pressure fluctuations. Do you feel that there is now sufficient knowledge of this aspect? The point really is, do you think we know enough about the forcing functions? For example, Prof. Lilley mentioned this morning the question of pressure gradients and things like that.

Prof. Lilley

It is fairly clear, from what I said this morning, that we are not quite clear about what happens in compressible and supersonic flows when we have no disturbances outside the layer. We require a lot more free flight measurements to check what has been done in wind tunnels. This is absolutely essential. When we go to supersonic speeds one of the new problems is that of heat transfer. We need to know what happens to wall pressure fluctuations in the presence of large rates of heat transfer to the surface.

Prof. Powell

I don't think we even have enough information at low speeds yet. If you think of the response of a panel at resonance, its bandwidth is something like 2% or 4% of the frequency. So it seems to me that we want to measure the space time correlations in band widths of about the same width, or certainly not very many times greater. We can always go from narrow band widths to broad ones by adding them together, but I know of no general way of deducting the correlations in narrow band widths from those in wide ones.

The information that we really need for the panel response is the space time correlations over the whole plane, for the general point, so that we can obtain the wave vector components in both directions and the convection velocity.

Now the situation isn't quite as bad as it sounds because we only need the time delay to the extent of plus or minus one half of a cycle at any given frequency. This is quite a modest time delay.

But until we do have these correlations in narrow bands for the general point, then we will be for ever making approximations without knowing what these approximations involve.

Prof. Richards

What do you mean by narrow bands? Do you mean that you take out the energy of a few cycles from the signals and correlate it, with a time delay, with another point?

Prof. Powell

Yes

Prof. Richards

We've been trying to do this and we have always been told that this isn't what is wanted, and that you can get all this from a full time-space series of correlations.

Prof. Powell

Well, I know of no way without making assumptions about frozen convection or something of this sort.

Prof. Richards

But does the frozen convection argument really come in when you are dealing with structural response?

Prof. Corcos

I think this information is essentially available.

Prof. Garrick

Our interest has largely been in knowing the input, the system that we have to deal with, and whether we can predict the output. Even with linear systems, with a phenomenon like buffeting, the input is highly three-dimensional. If we did measure a lot of these correlations we might eventually be able to crystallize something from them. But we have found that it is actually profitable to build a dynamic model and use the wind tunnel. In some cases this will produce the same kind of buffeting that you would get in full scale. On the other hand, in the case of the ground winds problem we have found that the wind tunnel is inadequate to give us the information that you get in the atmosphere. The exact reason for this is not entirely clear but I suspect it is because the power spectrum of the average wind tunnel is very widely different from the power spectrum of the boundary layer of the earth.

Now when we consider non-linear problems, with reference to fatigue questions, the amplitude itself becomes very significant. As Professor Mollo-Christensen said, we need information on the probability of large amplitudes for fatigue calculations. In connection with the high decibel levels we find that even aluminium panels become non-linear.

So my main point is that there is a whole spectrum of areas to be worked on and I agree fully with Professor Favre that we should pick our areas carefully. Probably the most useful thing one gets from a symposium of this kind is a knowledge of the problems not to work on as well, perhaps, as the problems to work on.

Dr. Strasberg

I would like to get back to the question of whether or not we have enough information. The information we have is about plane boundary-layers with zero pressure

gradient. Do we know enough about boundary-layers to be able to say what the characteristics would be on a surface which has either positive or negative pressure gradient? Many of the bodies we deal with do, in fact, have such pressure gradients.

Prof. Richards

Are the pressure gradients significant on a large body such as a submarine?

Dr. Strasberg

Well, the pressure gradients are certainly measurable and are significant for certain purposes. You're really asking the same question as I have asked. Are they sufficient to affect the boundary layer pressure fluctuations? I don't know.

Prof. Favre

As far as our measurement on the boundary layer with zero pressure gradient and with an adverse pressure gradient showed us, the basic phenomenon for turbulence and space-time correlation conservation is exactly the same. But the difference lies in the local mean movement, the average being made on time. This movement could be roughly two-dimensional when there is no pressure gradient, but this is not so when there is an adverse pressure gradient. Three-dimensional mean gradients are then appearing. This is the only difference; but for the basic properties of turbulence there is no difference.

Prof. Corcos

Does the intensity of the turbulence change?

Prof. Favre

Not much. It changes, of course, but the mechanism is qualitatively the same.

Dr. Strasberg

Would velocity measurements indicate the effect of a pressure gradient on the fluctuating pressure on the boundary?

Prof. Lilley

I raised this very question myself this morning. I feel that we want more information on the effect of wall pressure fluctuations in pressure gradient and we are working on this problem at the moment with a glider.

Prof. Ribner

Can I interpret the sense of part of this discussion to mean that the kind of information we've been getting is perhaps sufficient for many purposes, but that we need to get it under a wider variety of circumstances?

Prof. Richards

I think we want to get it in such a way that we are more sure of the low frequency and high frequency ends of the spectra.

QUESTION 5

What is the effect of Mach number?

Prof. Richards

Also we want to cover all sorts of non-uniform conditions, including, presumably, high Mach number conditions. I wonder whether we couldn't come to this topic now. It seems to me that we should really be getting a lot more data in supersonic conditions. Not only should we get it in the wind tunnels but we should also try to validate it in the case of very high Mach number flight tests. I wonder whether we might not have a discussion on that. Dr. Laufer, you've been working on high Mach number conditions. Would you like to hold forth on that? Then I would like to call on Dr. Ffowcs Williams to discuss the theoretical side.

Dr. Laufer

Maybe I can discuss it in terms of posing two questions to the panel members? Perhaps that will lead to some discussion.

The first one is the following: in what way is the source term in Lighthill's formulation changed when we go from lower Mach number flows to high subsonic and supersonic flows? Lighthill in his second Report concluded that as long as the square of the mean Mach number is not large the source term is well approximated by the double divergence of the Reynolds stresses. What new information do we need to estimate the source terms at higher Mach numbers?

The second question is this: if, indeed, the source terms change very much as the Mach number increases, is the present formulation useful? Would it perhaps be better to formulate the problem differently? I would very much like to hear the comments of the panel on these questions.

Prof. Richards

Well, who would like to give the answers? Professor Ribner?

Prof. Ribner

I think that a weak point in the formulation as used at subsonic speeds, when extrapolated to supersonic convection speed conditions, is that one still assumes that the density in the quadrupole term (if you are using that formulation) remains constant. You have a double time derivative involved and the fluctuations in density can now begin to contribute quite a bit and we don't know how much. Now in order to avoid this difficulty, O.M. Phillips reformulated the approach so that the stationary wave equation became a convected wave equation in which the convection terms arose from these density derivatives. They were taken from the right hand side of the equation and transferred to the left hand side in a reformulated form so that the whole thing could be recognized as the equation governing sound in a moving medium. In this formulation the density variations were very much less effective in the source term that resulted on the right hand side. And Phillips was able, in a rather idealized situation, to calculate the asymptotic radiation of sound for Mach numbers in the hypersonic range. It seems to me that this approach has considerable merit.

Dr. Ffowcs Williams

I would be the last one to think that the situation was well known at high speeds. But I think that we have to be very careful in discussing advantages of different theories because in my opinion Phillips' approach is precisely equivalent to Lighthill's approach at high speeds. The main term brought about by density fluctuations in the stress tensor is the one Phillips isolated. And this is precisely the one that is isolated by transforming into the moving axes and bringing out the Mach number down there.

It is significant that, when the same profile for convection velocity is put into the Lighthill and the Phillips approach, the answers are identical apart from a singularity in Phillips's integral. So I don't think that Phillips's technique offers any advantages whatsoever over Lighthill's in bringing out compressibility effects. Perhaps it would do if it were processed in a different way, but this hasn't yet been done so I don't think your point is a good one.

Prof. Ribner

If one uses the second time derivative of pressure in the dilation approach and assumes a convected pattern one can formally obtain the same results as from the Lighthill approach even up to the supersonic range. And if you assume that the pressure fluctuations before you introduce convection embody an eighth power law you obtain a modification due to convection. This modification at the higher supersonic speeds gives U^{-5} . Since this multiplies the assumed U^8 , you get a net result of U^3 . But, in effect, the U^8 law involves the assumption that you are neglecting the time derivatives of density in the source term. I don't see how you have retained the effect of density fluctuations in these alternative approaches.

Dr. Ffowcs Williams

Granted the problem is a difficult one but the point I'm trying to make is that you can bring in the convected wave equation, either in the differential equation form, in which case you do what Phillips did, or you can bring it in in the integral equation form, in which case you do what Lighthill did.

Prof. Ribner

But then you throw out the density derivative.

Dr. Ffowcs Williams

No you don't. This is precisely the point. The answers are identical. In the differential equation form Phillips can allow for variations in the speed of sound, but he doesn't. Lighthill can account for them in variations of the stress tensor, but he doesn't. At the moment the convected wave equation is solved in two ways; Phillips in one way, Lighthill in another. And they are both identical.

Prof. Ribner

I see. To carry the point one step further, is there an explanation for the difference between the final asymptotic power that Phillips obtained and the one you obtained from the extended Lighthill theory?

Dr. Ffowcs Williams

Certainly there is. If you put in the same velocity profile for convection velocity in both models they give the same result. You remember Phillips had some sort of Gaussian distribution. In Lighthill's form, which I used, there is effectively just one value. The difference in model is the main reason for the different result. But unfortunately the infinite plane result is singular because Phillips does have a singular problem to begin with. But the singularity is of the same order as that which would occur in Lighthill's if we used the same model.

Prof. Richards

Professor Lilley?

Prof. Lilley

I agree with Dr. Ffowcs Williams on this point, but if one is dealing with the problem of fluctuations at the wall, where one must take into account the variation both of mean density across the flow and fluctuating density, then I think it is preferable to use an equation of the form given by Phillips. This is the one that I have been using in working out pressure fluctuations at the wall. I agree that when you go over to the radiation field then Lighthill's formulation is the most direct way of tackling the problem.

Dr. Ffowcs Williams

Of course it is of value. I was not trying to imply in any way that it was not very valuable.

Prof. Ribner

I would like to make one more point in favour of the connected wave equation approach. It is the only way I can see that one can handle refraction and diffraction effects conveniently. Although, as Lighthill correctly stated, refraction, diffraction and scatter are implicit in the Lighthill integrand, the practical matter of injecting them is rather difficult. By reformulating the problem in the form of a convected wave equation you have a chance of calculating refraction. A number of people have done this for idealized problems, for example Gottlieb and Slutsky.

Prof. Richards

Now Dr. Laufer, what about the question you asked? I've a feeling you will have to answer it yourself.

Dr. Laufer

Maybe I could make a suggestion just to see whether I get any violent reactions from the others. In the exact expression of Lighthill's source term both density and pressure fluctuations are present. I suggest that the density fluctuations in the Reynolds stresses might not be very important. I base this suggestion on some of the work that Morkovin presented about a year ago at the Marseille Conference.

Essentially, he says, after looking at the boundary-layer measurements of Professor Kistler and also at some early measurements by Kovasznay and by himself, that there is very little interaction between the vorticity mode and the entropy mode at the lower supersonic Mach numbers. In other words the entropy mode is a fairly passive fluctuation field. Therefore, in place of the Reynolds stresses the term $\bar{\rho}u_i u_j$ could be used where $\bar{\rho}$ is the local mean density. So possibly by using this simplified form of the Reynolds stresses and by using the well known Stewartson-Dorodnitsyn length scale instead of the physical distance the first term in the Lighthill integral might be acceptable. On the other hand, the terms involving the pressure and density fluctuation $p - a_0^2 \rho$ will certainly contribute to the integral at higher Mach numbers; unfortunately I have no suggestions at present on how to handle these.

Prof. Kovasznay

What you really mean is that you can get the mean Reynolds stress reasonably well by that method. Whether you can get the sound generating term right is somewhat questionable. The case when there is no pressure gradient is not so difficult, but the picture is completely different when one has a pressure gradient because then the lumps with different densities tend to separate in velocity quite strongly. It is almost equivalent to the case of a no pressure gradient with the addition of a very strong body force proportional to density. I would make the guess that it would radiate as dipoles. The behaviour is most likely due to a body force, as is the case of a mild shock wave. I would suggest an experimental approach along these lines. The rough picture in a supersonic turbulent boundary-layer is that the entropy fluctuations are excited and eventually completely scrambled by the velocity field. You have a very analogous situation in a low speed turbulent boundary-layer with heating. In both cases, you get a typical 0.7 to 0.8 correlation between u (the longitudinal velocity fluctuation) and the entropy fluctuation. This is a very high correlation and, of course, also gives a high correlation with v because there is a correlation between u and v of the order of 0.4. In this case as soon as you subject the flow to a pressure gradient the different densities tend to separate out and one obtains almost the same result as if there was a gravity effect. You can simulate it by a gravity effect, and you obtain a very strong body force, or a virtual body force, on the lumps which would radiate as low as a dipole. I suggest that this should be done just in the case of low speed with a heated plate in order to obtain the information in a much easier experimental environment.

Prof. Ribner

Since you raised the point about entropy and the entropy lumps being scrambled, I would like to ask you whether you could suggest if the time variation is significant or not, because of course the time derivative of entropy is a source term. I would be inclined to think that this would be a strong source of sound under those circumstances.

Prof. Kovasznay

The Prandtl number is of the order of 1 or 3/4, so you don't get a time history of the entropy spots very different to that of the momentum spots, when speaking in rather broad terms. You see, my main point is that there are very big differences between a mean pressure gradient and no pressure gradient case.

Prof. Ribner

What I'm getting at is that it's the momentum fluctuations in the $\rho u_i u_j$ term that radiate sound. In addition, we have a double time derivative of entropy and I'm wondering if that could also be comparably big.

Prof. Kovasznay

My guess is that in the no pressure gradient case it won't be, and in a pressure gradient it definitely will be.

Prof. Favre

Dr. Laufer was waiting for a violent reaction and here it is. Neglecting ρ' in the product, the mean value of $\rho u_i u_j$, could such an approximation appreciate the magnitude by neglecting with respect to 1 the product of the triple correlation coefficient between ρ, u_i and u_j, times the intensity of turbulence of ρ, the intensity of turbulence of u_i and the intensity of turbulence of u_j? If the intensity of turbulence of the three terms is small, a few percent, then the triple product is zero, but if this is not the case then you have a magnitude of the term neglected.

QUESTION 6

Is research not going away from practical problems? What about turbulence investigations aimed at predicting the response of structures?

Prof. Richards

I would like to ask about the more practical problems or applications. I realize that the conference isn't actually called for this purpose but it is a noise conference and we have tended to ignore that side of it. I think the whole question of what to do next comes in here, for example the value of structural models and the testing of model structures and similar things. As a person who is interested in the outcome of this research, I think the thing that worries me is the feeling that our discussions are going away from the practical problems, particularly since the practical problems seem to be almost entirely related to structural movements in some form or another. I really wanted to ask whether the panel had any thoughts about models of structures, whether they had any plans to carry out experiments on structural excitations at all, or whether that part of the investigation isn't really being done. I don't know whether this is the sort of place to ask this question. Professor Lilley, I don't know if you have any thoughts on this?

Prof. Lilley

I think it would be very valuable to hear from somebody else, what they think of the value of doing structural response work in wind tunnels. Most of the information one seems to get at the moment which has any value seems to come from free flight work. However, as we saw yesterday, it is possible to perform some very valuable work in a wind tunnel, as was done at Toronto. I would like to know what other people think about the value of doing these structural response studies, especially on models of typical aircraft structures. I feel that we are getting near a point where we need something like a breakthrough in this area, and I cannot see it being done unless controlled experiments of the kind that can be performed in wind tunnels are completed.

Prof. Powell

I think this is very true, but perhaps more important than the structural models is to get more information about the forcing functions themselves for the practical structures. If you consider, say, a satellite launch vehicle, the wakes from the capsule and tower, the large changes of angle down the length of the cylinder and the various projections that are always there make the boundary-layer very different from those which are best suited for basic experimental investigations. I would like to see some more work carried out to investigate the flow characteristics, maybe in a more gross fashion than in these well behaved boundary-layers, to sketch out the main actions which are taking place and to try to understand the difference of the pressure fields in these areas. I feel that this is a step that must be carried on with the investigations of the structures as another and parallel item. The pressure levels at the wall of the space vehicles are quite different to those in a tunnel. The spectra shapes are very different too. There are wakes running along the boundary layer, various projections, shock wave interactions and problems of this sort. These nearly always turn out to be the problem areas. The areas where we have a very well behaved

flow are the areas we worry about least. It's the areas of 'dirty' flow, wakes, shock wave oscillations and so forth that are the areas that worry us most. I would like to urge the people with wind tunnels available to investigate these not very nice but very important boundary-layers.

QUESTION 7

Are the wind-tunnel tests useful? How about flight experiments?

Prof. Richards

I wonder if we could go back to the rather simpler cases of whether you can actually do tests on smallish models of structures, or do you have to represent a fairly large proportion of the structure. Presumably the only objections to wind tunnels are that firstly you don't have the right pressure differential, secondly that you can only put in a panel, or at the most two panels, and thirdly that you have standing waves on the low frequency side that may be incorrect. That's really what it amounts to, isn't it? One would think that the low frequency side could be coped with because you can have panels whose natural frequencies are higher or which can be made higher. I would have thought that tests in wind tunnels would be extremely valuable. The big snag is to keep the noise out if you're going to do transmission loss experiments. Professor Ribner, this is your real objection to wind tunnels isn't it?

Prof. Ribner

This is why we went to a duct, to get rid of all but the desired forcing functions.

Prof. Richards

But it does seem more sensible, does it not, to do more work in wind tunnels where you can put a large panel in the wall? However, are you going to get anything at all if the wind tunnel is an ordinary one? Is the noise level in a supersonic wind tunnel so high as to completely cloud any effects? There'll have to be special wind tunnels.

Prof. Favre

We have a special supersonic wind tunnel which is a very quiet one.

Prof. Richards

I think we do too, so we must talk to each other. We don't have one in which you can put big structures, however, and I think that this is the big difficulty. If you put in small structures in order to get excitation of significance, then you have very small movements with the attendant problems of 'oil-canning' and fixing.

Prof. Lilley

Can we have a comment from Dr. Garrick?

Prof. Garrick

Well, we believe that wind tunnels are still very useful. We have very long programmes on the structural response of aeroelastic models of launch vehicles for the buffeting phenomena in particular. We feel that the results are fairly reliable on the basis of whatever comparisons we can make. We're also interested in the very low frequencies, contrary to what you stated a little while ago. For example, for

the very large boosters a big range is actually sub-audible. To that end we're building a noise facility with a 14-foot diameter loud-speaker in order to study this part of the noise spectrum further. This is a part of the noise spectrum of which we know very little.

Prof. Richards

Yes, I think you misunderstood me. What I said was that if you are going to have low frequency phenomena on largish panels, then when you scale these to model sizes, you tend to scale them so that the frequencies you are interested in are fairly high. Consequently you probably don't have to worry about the low frequency standing waves that occur in a wind tunnel.

Prof. Garrick

Oh. Well, this is a different point. I'm sorry, I misunderstood you. Nevertheless what I said might be of interest. We're also very much interested in the fatigue problem which you mentioned so prominently. This problem is of course applicable to the supersonic transport. Any vehicle which has to have a long life, of the order of 10,000 hours or whatever it might be, has a very large fatigue problem. For the launch vehicles which have a short life, we're interested in the other end of the spectrum, that is, in the low-cycle higher-stress end that may design a certain part of the structure. Now the type of buffeting that excites local responses in a vehicle is the hardest one to duplicate in a wind tunnel. We can, however, readily duplicate the inputs that will excite the overall vehicle.

Prof. Richards

Could I ask you one question? It seems to me that we must have more measurements in flight testing to validate wind tunnel measurements or to validate whatever is done in model scale. Wherever we do have flight experiments they are inevitably clouded by some technical difficulty, or some technical limitation such as the frequency response of a microphone, or, alternatively, vibrations of panels, or something of this kind. Why is it that we never get good flight experiments?

Prof. Garrick

This is of course a rough question. I'm sure a lot of people here know the answer better than I do, but I would just like to mention one point and that is that in connection with launch vehicles it is extremely difficult to get any priority for flight load studies, or response studies, over the scientific pay load studies. You might say that we are always second or third class citizens when it comes to actually getting a place on the vehicle. Nearly all our studies have been this type of 'piggy-back' experiment. There has been no launch vehicle study that I know of which has been primarily made for loads investigation, and this is astounding really.

Prof. Richards

Is this the case in manned aeroplanes as well?

Prof. Garrick

No. For manned aeroplanes they've learned from long experience that a flight investigation of loads must be done. I think that with a modern aeroplane there is such a study in each case.

OPEN DISCUSSION

Prof. Richards

I would like to throw the discussion open now. I feel that amongst ourselves we've had a good discussion. I hope you've been listening to us. I think that in the time we have available, since we ought to stop in about 20 minutes, anybody on the floor of the house who wishes to make a contribution, or particularly wishes to elucidate some point, or to suggest a line of research, may do so, particularly if we get a good discussion from it. Who would like to start? Mr. Smith?

1. Internal Noise

Mr. Smith

From the point of view of an engineer, it looks to me as though we're still faced with a fairly broad brush approach to the problems facing us. I would like to mention two problems. First of all, if the considerable amount of research that has gone on does eventually offer some means of controlling turbulence to some extent, this will also be reflected on aircraft drag. I wondered whether the members of the panel cared to comment on the possibilities here. The second point is perhaps related to the particular problem of the internal noise level in a structure, as determined by boundary layer noise, about which we do seem to know a fair amount, now. This is a bit of a hare-brained suggestion perhaps, but would there be any sense in attempting with a scale model structure a generalized four pole parameter test to determine broad internal noise levels due to external excitation, and radiated outside noise levels due to internal excitation of some form?

Prof. Richards

As Philip Doak put forward in our joint Report this morning, there is a possibility of outside radiated noise being related to internal noise, but I don't think that there is much chance of structural oscillations, that is to say stress levels, being related to noise levels because such a large proportion of the vibrations of a structure are non-sound producing. Therefore it is highly unlikely that you will ever have any simple relationship between internal noise and structural stress levels, nor between radiated noise and the drag of the aeroplane.

2. Simulation of Boundary-Layer Flow

Mr. Wills

On the question of the testing of model structures under turbulent boundary-layers, I should like to ask Professor Lilley if he thinks there is any advantage to be gained from using a turbulent wall jet to simulate a boundary-layer type flow over the structure. At least one would not be troubled by the large low-frequency fluctuations that seem to plague tunnel experiments.

Prof. Lilley

One of the problems with the wall jet, which we have studied fairly extensively, is that depending on the speed of the jet, one can get very large values of pressure fluctuations, and the spectrum has significant differences, although under certain conditions it is similar to that in a boundary-layer. Also, there is the problem of the mean value of the dynamic pressure associated with the maximum in the velocity distribution. This varies with distance along the plate. Therefore, I would say that under certain conditions this might be an acceptable model, but in general you would find it rather difficult to represent the phenomena completely. Certainly for structural testing it might be noted that there are other ways of getting the appropriate full-scale excitation than by using boundary-layer flow over the model structure.

Prof. Richards

In the context some of the experiments we are doing at Southampton University are of some interest in the sense that we feel that we don't have to represent the boundary-layer type of excitation exactly. We think we have to have a convected flow pattern and also the type of correlated pattern that occurs in boundary-layers, but we don't think we need a boundary-layer as such. We're trying out a scheme by which we're just making a very turbulent flow in a small two-dimensional duct, a very thin duct, in which the whole thing is stirred up rather like a pipe flow, with a view to trying to use the air available to stir up the panels as much as possible. By doing this we can use a much bigger structure than we would if we had a small boundary-layer in a large flow. Bearing in mind the similar sort of response that one gets for different types of excitation, if the correlation area is of the same size as a panel size, this may possibly be a method of testing larger structures and obtaining the radiated sound from them. It seems to us that we must, in fact, have structures which are not just panels, but that you must represent terminations of the convecting waves at the stringers on frames.

3. Response of a Panel

Prof. Mollo-Christensen

I wonder if the Panel could give an opinion, or several opinions perhaps, on the effect of panel response upon the pressure fluctuations. Is it sufficient to know the panel response in the absence of airflow and the pressure fluctuations under a turbulent boundary layer over a rigid surface to determine the response of a flexible panel subjected to a turbulent boundary-layer?

Is there an effect upon the structure of a turbulent boundary layer when it flows over a flexible wall?

We know that a laminar flow may excite panel vibrations. I wonder when it is possible to make a distinction between panel flutter and turbulence-excited panel vibrations. The mixed problem is of course difficult to analyse. It would be interesting to hear some opinions on this.

Prof. Kovasznay

I would like to say a word on that. If the panel is compliant enough to cause a fundamental change in the flow I can easily visualize intermittent transition on the panel. We know from experiments that flow in transition can radiate an awful lot more than a fully turbulent flow with no pressure gradient. Flow in transition can produce local sources that far exceed it. I am mostly referring to early measurements by Morkovin, who measured the radiated sound field in a supersonic flow. I have also recently done some low speed work measuring the detailed flow in transition and we have crudely integrated the equations to see what kind of pressure you get on the surface, and in fact you obtain quite spectacular pressure pulses there.

Prof. Garrick

May I just add a little comment? There was one case of a fighter aeroplane which was so noisy that the pilots almost refused to fly it. The noise problem was cured by thickening a few panels which had been fluttering. They were thin panels and it was a case of flutter, not a noise problem. Another point that I might make (Professor Mollo-Christensen did stimulate some ideas here), it is not only the panel but a panel with its attachments and discontinuities that is very significant, because the transmission of noise through a panel is greatly affected by the discontinuities. I believe some very significant work has been done on this by Richard Lyon, Gideon Maidanik, and Preston Smith of Balt, Beranek and Newman, Inc., which showed that the discontinuities in the panel are really the source of the noise.

Prof. Richards

This was in fact the work that I was referring to when I said that it wasn't just the panels but that it was the panels with the stringer and frame attachments that really matter. With regard to the problem as to whether or not there is a reaction back onto the boundary-layer, I can see that there may well be in the transition region, and one can also imagine panel oscillations occuring due to those movements. We've been trying to see whether the turbulence changes with a fine surface, but so far we haven't found very much difference in spectrum with a flexible surface. Dr. Dinkelacker, who is visiting us from Germany, is doing this work and he has something like a 1½ decibel variation in the turbulence with and without the compliant surface. There doesn't seem to be any large effect which is likely to alter the results in the practical case of an aeroplane with a fully developed turbulent boundary layer.

Dr. Ffowcs Williams

What about the spectrum? There could well be a small energy increase at certain frequencies which have important effects, yet the overall energy level may remain virtually unaffected.

Prof. Richards

This is the point. There is the odd difference of 1 or 2 decibels or so, but that's about all. However, there are noticeable changes in spectrum slope.

Dr. Ffowcs Williams

The natural plate vibration, whose scale might be much larger than the turbulent scale, might couple back again in a much more important way.

Prof. Richards

I think it's true to say that we may well not have the right conditions of compliance and damping, but so far we haven't achieved very much on this score.

Dr. Strasberg

It may be difficult in an air flow to get a surface which is compliant compared with the fluid, but in water it's very easy to get surfaces which are compliant with respect to the water. There are certain circles investigating the effect of compliant surface coatings. Their effect on transition from laminar to turbulent boundary layer is something of a controversy at the moment. There are some people who claim that there is a significant effect on drag in water flows. If any member of the panel has an opinion, I'd like to know about it.

Prof. Richards

I should have mentioned that our work was being done in water really to investigate the Kramer argument, but we felt it was more sensible in the light of experience to see whether the spectrum changed with a compliant surface rather than to try to stabilize a laminar flow.

Dr. Laufer

This is pressure that you are talking about?

Prof. Richards

Yes, this is surface pressure.

Dr. Strasberg

Do I get then from your comments that the effect that you observed, if it exists at all, is very small?

Prof. Richards

Yes. So far it's very small, but I wouldn't like to say more than this.

Dr. Laufer

I would like to say that we are thinking along these lines, not in water but in air and in supersonic flows, where we hope that one might be able to use surfaces which do react the right way under a turbulent boundary-layer. We are not trying to change the transition Reynolds number of the boundary-layer to keep the flow laminar but just to see whether there is any possibility of reducing the turbulent energy in the boundary-layer.

Prof. Lilley

Could Dr. Laufer give us any information on what kind of surfaces he is using?

Dr. Laufer

We are working with extremely thin membranes that are of the order of one thousandth of an inch thick, which are then supported by various types of structures. This is just a first step. At the moment we're just playing without having any results.

Prof. Lilley

Can I just come back on that one? We have also played on this for a year at low speed without any success.

WHAT ARE YOU GOING TO DO NEXT?

Prof. Richards

I think it would be very useful to just run along the panel and simply say 'What are you going to do next'? This is a hard question, but it is an interesting one. If I may I would just say that one of the things that we are going to do next is to investigate this problem of structural behaviour, since every noise problem arising from boundary-layer pressure fluctuations involves structural response in some way. We feel that this is something we must get into and that this is the sort of thing we feel is more important. Also we shall do some basic work on turbulence. Dr. Davies is very keen on doing some work in a jet and I think he's right. I feel that is the sort of thing we shall do.

Prof. Favre

As I said a moment ago, we shall continue the investigation concerning the structure of turbulence in the boundary-layer with a space-time correlation and with filters in order to get some more description of the big eddies and maybe a correlation with intermittencies. We are also doing the same work for flat plate boundary-layers with suction and also with heated plates to get the heat transfer by turbulence. Now we are beginning to join these together in order to have both suction and heating to get the Reynolds' analogy with the diffusion of mass in turbulent boundary-layers. We are also doing the same things with cylindrical tubes instead of flat plates, using heating, suction and both heating and suction. About one year ago we began to work on supersonic turbulence and now it's beginning to work using a supersonic low turbulence tunnel. We are also beginning to work on the wakes on a longitudinal cylinder in supersonic flow.

Dr. Ffowcs Williams

At the moment I'm working on flexible boundaries and I'm trying to use an energy approach to see if I can find any effect theoretically on the question of transition and critical Reynolds number. I'm trying to look at something which may become apparent without the effect of coincidence of wave speed and frequency. The poor old dolphin doesn't know he has to move his surface waves and it appears so far that the stability analysis doesn't apply to the dolphin problem, if indeed that problem has anything at all do with stability. On the high speed side I hope to look at the rocket noise problem, the question of where the noise sources may or may not be located, the effect of variable convection velocity, particularly in the rapidly decelerating flow through the shocks from the nozzle exit down to the subsonic regions, the question of what the tensor may look like and what the source term may be, particularly those associated with entropy changes. I didn't get a chance to say it but experiments at the moment seem to show that if you have burning in the exhaust of a rocket then the noise goes down instead of up. The effect, quite contrary to the one we might expect, is very odd and quite unexplained. There will be theoretical work there that I hope to look at.

Prof. Garrick

We are going to continue work on buffet on a more systematic basis something like Dr. Kovasznay has mentioned. We're going to try to get correlations. We're going to measure the input so that we'll know what we have and then we'll try to predict the output. In other words, more systematic tunnel work on buffet is planned besides the overall work on aero-elastic models that we have been doing. We are continuing work on the fatigue of panels in an acoustic environment under various temperature situations, that is in the high intensity noise area. We are also planning to do some work on compressor inlet noise, which we think is an important problem. We have a continuing program on the sonic boom. Recently, a small program in this area dealt with the effect of the sonic boom on light aircraft.

Prof. Kovasznay

I'm continuing some work on transition and the understanding of the interior mechanism of transition. Noise appears to be only incidental in this problem. I'm toying with the idea of having another look at the heated low speed plate.

Dr. Laufer

To be quite honest, in the problem of supersonic boundary-layer radiation as far as experiments are concerned we would like to sit and wait until we get some new ideas. One thing I would like to look at from the analytical point of view is to see whether for the supersonic case it would be possible to make up a simplified model as far as the source is concerned and to try to explain the statistical properties of the far field. As far as the experiments with flexible walls are concerned, we haven't given up yet and we shall try to see whether somehow we could inhibit the turbulence field near the wall in such a way that the turbulence production would decrease.

Prof. Lilley

Obviously from what we have heard at this meeting the first problem I will attempt is to satisfy myself that the calculations we made on wall pressure fluctuations are right. The other work that is in progress relates to the problem of the generation terms in aerodynamic noise problems. We note the concern about the dominant source terms at supersonic speeds but as far as I am concerned there are still many problems that require to be resolved at low speeds. There are more data available now than when I originally worked on this problem and I think one can go a bit further than I did originally. There are still some problems on wall pressure fluctuations at supersonic speeds to be worked out with the refinements and ideas that I presented at the meeting and I am still working on these. We are looking at the problem of the structure of turbulent shear flows with blowing, which seems complementary to what Professor Favre is doing with suction and heat transfer. Again on wall pressure fluctuations, we are looking at problems with pressure gradients and problems arising from separation.

Prof. Powell

I'm hoping to do some more work on the vortex approach, particularly on some fundamental problems of flow fields due to vorticity. One thing that I hope to start

very shortly is the question of jet instability in terms of vorticity. This will throw up the radiated sound problem at the same time and I hope to modify these theories to somewhat higher Mach numbers than now applicable. This is also relevant to some of the boundary-layer pressure problems. I would also like to follow further the resonating flow problems due to cut-outs and steps. This type of question, which is very interesting from a fundamental point of view, is also very important for the practical problem with so many of the ugly-looking aerodynamic devices that we just have to accept as facts. The other work that I'm very much interested in is following up the panel response work, in particular the influence of the shape of the correlation functions in the general direction and also to investigate various types of wall structure beyond the simple single skin with or without stringers to double walls and modifications of that. I think that will keep me busy.

Prof. Ribner

We have initiated some work on hot-wire space-time correlations in a jet as a graduate student programme, a rather too little and too late sort of thing in view of what we found being reported on at this meeting. The first thing I will be doing is re-examining our goals in the light of what has been reported to see what we should be doing in that regard. As for theoretical work, I have been considering lately some work on jet noise theory in which there is the possibility that as you go downstream from the mixing region into the transition zone the dominant noise radiators may be those near enough to the centre to be convected considerably faster than half the nozzle velocity, since you do encounter velocities of 0.8 of the nozzle velocity in there. This seems promising in appearing to explain certain anomalous changes of the spectrum shape with direction from the axis and the apparent lack of Doppler shift on the peak frequency. I am giving a paper to the Acoustical Society in New York on the early thoughts along these lines but I think actual calculations with more data need to be carried out.

Dr. Sternberg

I think mostly I'm going to content myself with watching Dr. Laufer in his investigation of the effect of a compliant wall on a turbulent boundary-layer, because I'm very interested in fishy stories. I think the question as to whether the movement of the wall can effect the friction of the turbulent boundary layer is very intriguing. It seems to me that the only way to make the dolphin work is to do something at the wall to somehow decrease the turbulent energy level across the whole turbulent flow. We can't just decrease the turbulence level right near the wall because the turbulence level and shear stress there is really associated with the vorticity field through the whole boundary layer. So the question arises as to whether there is some way in which the movement of the wall can act as a turbulent energy drain, perhaps increasing the flow of turbulent energy towards the wall and dissipating it there.

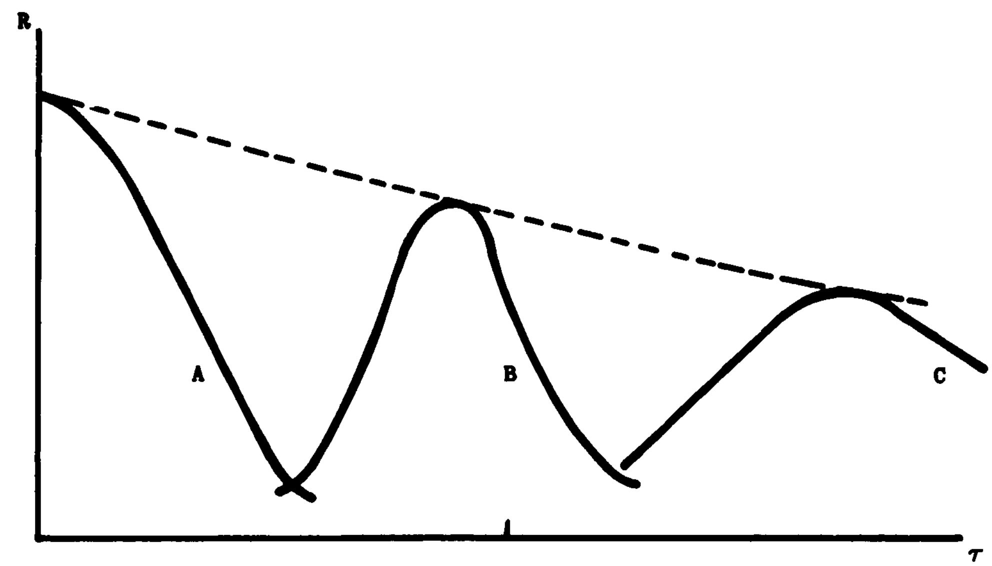

Fig. 1

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